SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES IN THE MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Anatomico-Physiological Remarks on a Person who had been Beheaded.*—Some very curious and interesting remarks on a person who had been beheaded are recorded in *Müller's Archives* for 1838, by Professor Bischoff, of Heidelberg. We extract the following particulars from a translation by Dr. Bigger, published in our esteemed cotemporary, the *Dublin Journal of Medical Science*, (Sept., 1839.)

The individual upon whom the following observations were made was a robber and murderer, named Sebastian Zink, who was publicly beheaded on the 6th of July, 1838. Professor Bischoff and Dr. Heerman stationed themselves immediately under the scaffold, so that they could have the head and body of the decapitated man immediately under inspection, whilst Dr. Jolly remarked on the outside every thing which should occur, the greater part of which could also be seen and heard by his two colleagues.

"A few seconds," says Prof. B., "after the blow with the sword had been struck, at thirty-six minutes and a half past nine, we received the head, which was hastily handed down in the bandage which had covered the face, without having been deranged by falling or receiving any shock. The bandage was immediately removed, in order to see the manner of separation from the body, the bleeding, and the impression of the features. The blow had been given most successfully, and the blade had passed anteriorly between the os hyoides and the larynx, posteriorly very nearly between the fourth and fifth vertebræ, yet in such a manner that a portion of the left oblique process, and of the body of the fourth and a piece of the oblique process of the fifth were hewn through. The blood flowed slowly and continually. The expression of the countenance was of the most perfect tranquility, not a trace of pain or distortion. The eyelids were a little sunk, the mouth closed, yet easily opened. The expression of the eyes was perfectly tranquil, neither staring, as mentioned by an earlier observer, nor yet dull and without lustre, but like those of a person who looks at something at a great distance. Dr. Heerman, who had visited and sat with the criminal for an hour on the preceding evening, could not discover any greater change in the features than the absence of his accustomed piercing, sharp glance, and the pupils being somewhat dilated.

"I approached my fingers, and then a brilliant needle close to the eyes, yet without touching either eyes or eyelids. This was repeated often without causing the slightest motion in either the eyes, eyelids, or other features.

Dr. Heerman now shouted his name in his left ear, and the word 'pardon.' under the conviction that this sound would produce an effect upon whatever consciousness might be present, as up to the last moment the criminal had been continually hoping for a reprieve. There was no trace of any motion. I now held to his nose a bottle of very strong smelling tincture of assafœtida, which preparation I preferred to ammonia, because I expected a purer action from it on the olfactory nerves, as it might be possible that the ammonia, by stimulating the nerves of the mucous membrane, should produce a motion of reflection. This experiment also failed. I then dropped upon his tongue a drop of the tincture of coloquintida; a feeble thrusting forward of the tongue and slight motion of the jaw ensued; both these motions were repeated three or four times, at short intervals, whilst in the other features no trace of motion was visible. In order to try, in a certain degree, how far the spirits of wine contained in the tincture could be regarded as cause of the motions, I placed a drop of pure spirits of wine on the tongue, which caused a similar motion of both tongue and jaw.

"All these experiments were finished within one minute after the fatal blow had been given. The question now arises, were those feeble motions of the tongue and jaw the consequences of sensation? if so, consciousness must have been present; or were they reflected motions dependent on irritation of the mucous membrane of the tongue, or from irritation of the severed spinal marrow, and therefore not caused by the tincture of coloquintida? To both of us the last of these suppositions appeared most probable. The tranquil condition of all the other muscles of the face appeared to contradict the idea that any unpleasant taste or flavour had caused these motions. The symptoms of the reflex function, however much developed in the motions of chewing and swallowing, are much less affected by irritants applied to the tongue than by those applied to the mucous membrane of the palate, uvula and jaws. I must mention, also, that real irritation of the mucous membrane of the tongue with the point of a pin produced no motion. On the other hand, opening and shutting of the mouth are the motions which are most frequently to be seen in decapitated animals, and which are certainly dependent on the violent irritation of the spinal marrow. The tongue can move very little at the same time, on account of the attachments of the muscles which retract it having lost their fulcrum.

"Thus there does not appear to have been a single symptom present which permits us to suppose that consciousness was present; it appeared to have ceased instantaneously. It is right to remark that previous to the blow being given, he was not at all confused in mind, as were the ten criminals beheaded by the executioner Brand, as communicated by Heim. Zink, previous to execution, was in a perfectly careless mood, yet a little excited, and displayed the lamentable rudeness of his general demeanour; his mental powers were so little affected, that he said to the attendants of the executioner, as they were binding on the bandage on his eyes, an instant before he received the blow, 'Ye slaughter one like butchers!' During life this man seemed to possess very little nervous sensibility, the same was evidenced after death, in the experiments performed, so the evidence of those who have obtained contrary results is not by this instance much weakened. Yet I may say that negative results of an observation in the state of mind in which the greater number of observers must be at such a moment, carry with them more credence than positive ones, in which the excited fancy may play a prominent part, as from the relations of others, who during similar operations previously have beheld marvellous things. All the evidence brought forward with any accuracy to prove the continuance of consciousness have consisted mainly in a few remarks on impressions being conveyed to the auditory apparatus in animals which had been irritated, which collectively cannot be considered as motions of reflexion, or as caused by irritation of the spinal marrow.

"I now proceeded to perform certain experiments from which I expected real motions of reflexion. I touched with a needle the eyelids and eyelashes as well as the conjunctiva; I irritated the mucous membranes of the nose, mouth and pharynx, in the expectation of perceiving motion, but all in vain; all the muscles remained perfectly quiet, so that this was a proof not only of the rapid disappearance of consciousness, but also of that of the nervous irritability. Even piercing the severed part of the spinal cord with needles, and touching it with a pencil of kali causticum, produced no motions more in the head, whilst yet not more than from two to three minutes had passed from the period when it was severed from the body.

"We now turned our attention to the trunk, and whilst Dr. Heerman quickly tied the carotids, out of which the blood continued to flow with little jerks, in order to support the nervous and muscular irritability by detaining the blood, I sought, by piercing, scratching and pinching the skin on the soles of his feet, fingers and toes, to call forth some reflex motions; this proved vain also. Irritating the spinal cord produced twitchings in the pectoral muscles, and elevation of the arms.

"The corpse was now placed in a coffin and conveyed to the neighbouring hospital, where every thing was ready for further experiments. It was now fifteen minutes past ten, consequently thirty-eight minutes and a half since the execution.

"We next introduced some electrical streams into the nerves. For this purpose Dr. Jolly had constructed a galvanometer, which by the immersion of a single plate of one-fourth inch square in pure water, produced an aberration of the magnetic needle to about 90°. I first sunk the platina terminations of the two conductors, one in the gray, the other in the white substance of the spinal marrow of the trunk. This process being repeated often, and the needles changed, not the slightest motion of the magnetic needle occurred; the introduction of the needles also caused no twitchings of the muscles. This experiment was tried on account of the assertion of Folchi, who stated, that in a calf which had just been decapitated, and on which a similar experiment had been tried, there was an aberration of the needle about 6° west, on every new introduction of the needles, (Froriep's Notizen, No. 950,) which experiment had often before been tried without any effect on decapitated dogs. Dr. Heerman now exposed the median nerve in the upper arm, and I plunged the two needles into the trunk of the nerve, at the distance of about one inch from one another, and endeavoured, by mechanical irritation of the spinal marrow to produce twitchings in the arm; none occurred, nor were there any changes in the magnetic needle. I now applied one pole of a powerful galvanic battery of sixty pair of four inch plates, capable of giving me a tolerably strong shock, producing sparks, and decomposing water, upon the spinal marrow, the other on the hand. There arose evident yet slight twitchings in some of the muscles of the fore and upper arm, viz: in the supinator longus, extensor carpi, ulnaris longus, and in the internal head of the triceps, but no motion in the magnetic needle. It also remained perfectly unmoved when I applied the second pole upon the trunk of the nerve, the same was the case when I applied both poles to the trunk of the nerve, so that the chain was perfect, although in the latter instance twitchings occurred in the muscles mentioned above. Some inference may be drawn for the hypothetical electrical streams in the nerves, at least that the nerves are exceedingly good conductors of electricity, and much better than the metals, or otherwise the magnetic needle should have been affected in the last case, had it been the better conductor. Mattevic, in the Bibl. Univers. de Genéve, August, 1834, has communicated a similar experiment with like result. On the contrary, J. Müller has, in the Archiv. für Anatomie, Physiologie, &c., declared that all parts of the body, and even a drop of water act in the same manner. Without wishing to raise any doubt against this observation, we may say that from it cannot be deduced that the nerves and perhaps other moist organised parts are not good conductors. The want of reaction in the galvanometer, when the circle is completed, by any of these parts, will admit of the general conclusions being drawn either that they are very good or very bad conductors of electricity. The experiment of Müller admits only the one explanation, that the moist organic structures are very bad conductors, for he only employed a weak source of electricity, a single pair of moistened plates, and the circle was not completed, and thence there was no aberration of the magnetic needle. On the contrary, in our case the occurrence of reaction, the contraction of the muscles showed that the circle was completed. Should any one object that nerves are bad conductors, and that the results we obtained were from our using a very powerful source of electricity, his own argument might be used against him, for the more powerful the electricity, the more likely would it be to pursue the metallic conductor if it were the best. The non-reaction of the galvanometer appears to have been caused by the very great conducting susceptibilities of the nerves (or perhaps of the other animal tissues,) for electricity. I do not wish to draw as a conclusion that electricity is the active agent of the nervous system, a circumstance which for many other reasons I think to be improbable. I have repeated also the experiments of Varvasseaux and Berardi, as well as those of David, and in no instance has there been any change of position remarked in the magnetic needle when I sink the platina conductor of the galvanometer into the nerves of a living animal, and by means of mechanical irritation caused motions and twitches, actions of nervous irritability. This is accounted for, as we said before, from the nerves being better conductors than the metals.

"Mechanical irritation of the median nerve, pricking, pinching, and even cutting it across, produced no effects, whilst galvanism still produced twitchings.

"I next opened the chest and abdomen, whilst Dr. Heerman tried some experiments on the irritability of the iris. It was now fifty minutes past ten, and the iris showed no disposition to contract, not even when the pole of the battery was applied directly to it, the cornea having been removed, yet the battery was so strong that it immediately caused decomposition of the fluids of the eye, and a development of gas.

"In the chest and abdomen there were no spontaneous movements to be seen; also when the galvanic pole was applied consecutively to the phrenic and vagus nerves, to the stomach and intestinal canal, on the ureters, gall bladder, and the cystic duct, no contractions occurred. The right auricle of the heart alone continued to move after this, for an hour and a quarter, the same occurred in the muscles of voluntary motion, but was scarcely perceptible after an hour and fifty minutes. As the experiments on the phrenic and vagus nerves were made at ten minutes past eleven, just one hour and thirty-three minutes and a half from the execution, even then the twitchings in the voluntary muscles were very weak, in comparison with those mentioned by other observers in other cases, which confirms the remark which I made already with regard to the head, that the nervous faculty and capability of irritation were not strongly developed in this man.

"Whilst these observations were making, I was seeking the thoracic duct, which I found, with a good deal of trouble, on account of the body being very fat and the vessel containing only a muddy, grayish fluid: a portion of this fluid being removed, coagulated as usual, but it formed a very small coagulum around the bit of stick which was moved in it. Under the microscope the little round bodies did not appear numerous in the fluid, but were nearly of the same size as the molecules in the blood, many of which might have entered along with the chyle whilst we were obtaining it, but there is sufficient to distinguish these two sets of bodies from one another.

"I take advantage of this opportunity to communicate my observations on the chyle molecules, which I had often examined in dogs. In the white chyle of the thoracic duct of young dogs, after being fed, I have always seen two kinds; the first exhibits an innumerable quantity of exceedingly small, little bodies (kòrnchen) or particles, which are only to be seen when the chyle is permitted to run on plates of glass, when their appearance is exactly like that of sand put in motion. The microscope must be very powerful to show this. The second kind of little bodies are fewer in number, much larger, and the greater part of them of the size of the moleculés in the blood, but there is no kernel and no envelope to be seen in or on them, besides they have not the yellowish appearance which, when powerfully magnified, even a single blood molecule exhibits; they are unchangeable in water and in active acid. When the chyle is agitated they sink to the bottom, and they form a part of the coagulum. The rest of the fluid remains white, where they are in very small number, and where they could not be the cause of the intense white colour. This colour is, I think, attributable to the other set of innumerable little molecules, which I take to be fat. Still I am opposed to the opinion of J. Müller, that the white colour of the chyle is principally attributable to fat; for if the chyle be treated with æther, in a glass tube, often shaken, and fresh æther still added, (not in a watch glass, where the experiment cannot succeed,) it loses nearly all its colour, and appears of a light opal. This remnant of opacity cannot be caused by the larger molecules, for they were dissolved just as the blood molecules are by the æther, a process which I saw going on; but it seemed to me that this opacity arose from coagulated albumen, which is more difficult of solution in æther. Very numerous little bodies were to be seen in the fluid, which remained like coagulated albumen. I believe that the evidence of Tiedeman, Gmelin and J. Müller are alike, all considering the prevalence of the white colour in the chyle as proceeding from very minutely divided particles of fat, but that these are not the genuine chyle molecules which exist independent of them. This view is strengthened by this, that in the pinkish contents of the duct in dogs which had fasted long, those very fine particles were not so evident, whilst the other chyle molecules were even as numerous, nay, relatively more numerous than usual.

"After having collected the chyle, I observed the mucous membranes of the trachea and larynx, and had the pleasure of seeing myself, and being able to show to those present, the ciliary motions. These motions were not discoverable in the æsophagus, not even in that part which covers the posterior wall of the cricoid cartilage.

"I next examined the urethra, vesiculæ seminales, vas deferens, epididymus. In the urethra it was evident that an ejaculation of semen had taken place, as in the case given in Valentine's Repertorium, I. p. 277; this arises, naturally, from the violent contraction of all the muscles at the moment of decapitation. There was found in the urethra many large, yellowish coagula, like coagulated lymph, and a whitish fluid which contained mucous seminal animalculæ alive, but not so numerous as those which I have seen in the seminal fluid of other animals. The same kind of coagula, and the same animalculæ were seen in the vesiculæ seminales, which were of small size. The animalculæ were found in the whole course of the vas deferens, alive, but not in such great numbers. I thought I could see them also in the seminal canals of the testis, but as they were few in number, and not living, I do not wish to assert this as a positive fact. The contents of these parts contained also many other kinds of particles, but none of such determined forms as those which Valentine describes. It is probable they were particles of epithelium. The fluid which exuded, on a section of the prostate being made, was clear and pellucid, but I could not discover in it any elementary bodies, except some blood molecules.

"Much pains were taken to examine the brain carefully; when the skull-cap was removed, all the veins were found to be full of air, and air had penetrated between the pia mater and arachnoid, causing a very peculiar appearance on the surface of the hemispheres. Could this air have entered at the severed part of the spinal cord? This is difficult to credit, for in the brain as well as in the spinal cord, the pia mater and the arachnoid lay very close together, and no fluid intervened, the escape of which might permit the entrance of air. Through rupture of the vessels? This I am not willing to allow either. And by this my view is strengthened, that the formation and course of the arachnoid is not yet perfectly discovered. The examination of the brain was not satisfactory. Particular attention was paid to the spleen: in it I found the little white bodies described by Malpighi, which I have seen in the four classes of vertebrated animals, but which are hard to discover clearly in man. I believe that the function of the spleen is to produce them, and I cannot see any reason why Müller should suppose that those he has seen in certain ruminating animals, are of a nature different from those which have been found in the spleens of other animals, or from those which I believe to exist in the healthy spleens of all men. That these are hard, and tolerably large in some, whilst in others they are soft, small, and easily dissipated, is no reason that they should be considered essentially different. I found, also, in these bodies, universally, as well as in the spleen of this subject, those little round bodies or balls which J. Müller has described.

With a cataract needle it is easy to isolate as many of these little bodies from the spleen as may be necessary for microscopical observation. I find that these are exactly similar in appearance, size, relations to water and acetic acid, to those of the chyle; therefore is it not possible that these may originate in the spleen? It is true that the corpuscules of which the parenchyma of the liver consists, are, as Müller remarked, very like those in the corpusculi Malpighiani, yet similar bodies are to be found in many places, and still the parts organised from them are very different from one another.

"From attention to this point, I think shortly, much information will be obtained with regard to the spleen, and the formation of the corpuscules of the blood, although as yet no change has been seen in the formation of blood after extirpation of the spleen. It is questionable whether sufficiently accurate examination has been made of the blood globules in those animals which have been deprived of their spleens.

"Lastly, I examined the stomach and the villi of the intestinal canal. In the former the mucous membrane of the fundus was strikingly softened; it was now 4 o'clock, and the day very warm, the contents of the stomach smelt very sour, from wine and salad. The villi of the small intestine were of pyramidal form, formed of little bodies united together, and I could not discover any epithelium formation on them after they had been gently washed in water.

"There was no diseased appearance in the whole body, except an adhesion of the lung to the walls of the thorax, occasioned by an old gun-shot wound in all probability; three grains of large shot were found under the pleura costalis, unchanged in appearance, and surrounded by a quantity of cellular tissue.

"The ventricle of the heart was very strong and muscular, and compared with the rest of the muscular structures, might be called hypertrophied.

"After six hours busily and uninterruptedly employed with the body of this unfortunate man, though aided by my friends, I still feel that many observations might have been made in a more accurate manner. My hope is, that what has been done may lead to further observation on the same subject."

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2. *Ectopia Cordis.*—Two cases of this rare mal-formation have been already recorded in this Journal, (see No. for February, 1833, p. 346, and No. for November, 1838, p. 192,) and we have now two more to add to them, extracted from recent foreign journals.

Case 1.—On the 11th of February, 1838, Dr. PECCHIOLI, Professor of Clinical Surgery in the University of Sienna, was sent for to the *Hôpital Santa Maria della Scala*, of Sienna, where a child had been brought, born at the full period, the heart of which was exterior to the chest. This organ had protruded through a nearly circular opening of about an inch and a half in diameter, the margin of which was regular. The pericardium was entirely wanting, the heart appeared naked. The coronary vessels well developed and full of blood were easily distinguishable. The large vessels, both arterial and venous, seemed as if prepared by the scalpel of the anatomist; they passed into the cavity of the chest, leaving the heart hanging like a medal. This organ could be seen in the performance of its functions; the right cavities were anterior and the left posterior. The apex of the heart was turned downwards, and in the contractions of the left ventricle, it could be seen to bound forwards, and move more than an inch from the points at which it was during its period of relaxation.

At the moment of birth, the action of the heart was energetic, but it gradually lessened until death, which took place in 26 hours.

The autopsy showed that the upper piece of the sternum was deficient, except a small lateral portion to which the clavicles were articulated. The xyphoid cartilage was present.

The specimen has been preserved in the museum of the hospital.—*Gaz Méd. de Paris*, 5 Jan., 1839.

Case 2.—On the 20th March, 1839, at Schneidemühl, a stout woman, who had before borne five living and healthy children, was delivered of a remarkably stout female infant, presenting the following defective formation:—The heart, and under it, the stomach, both organs apparently separated by a partition, lie outside the thoracic and abdominal cavities, in a sack of skin nearly transparent. The protrusion is through a deficiency of the lower third of the sternum and upper part of the wall of the abdomen, as far as midway between the pit of the stomach and navel. The fissure is altogether 5½ inches long, and 2¼ inches broad, and is situate almost in the middle line of the body. The child is living, sucks, and is otherwise well. The prolapsed parts are protected by an appropriate bandage.—*B. and F. Med. Rev.*, Oct., 1839, from *Med. Zeitung*, April, 17, 1839.

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3. *Double Peritoneal Sac.*—M. Nonat communicated to the *Société Médicale d'émulation*, a curious anomaly in the peritoneal sac observed in a man æt. 60, who died in the Hôtel Dieu, of gangrena senilis. On opening the abdomen there was found in addition to the ordinary peritoneal sac, a second one, concentric with the first, and enveloping the small intestines as far as the ileon. This second peritoneum was identical in its structure with the first, and appeared rather congenital than accidental.—*Gaz. Méd. de Paris*, Sept. 7, 1839.

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4. *Experiments on Digestion.*—The following are the results of some extremely interesting experiments of Professor Schultz, of Berlin, instituted for the purpose of determining the relative digestibility of food in the stomach, and to ascertain the nature of the movements of that organ. The experiments themselves, from which these results are derived, we shall give in our next number.

"The first result," says the Professor, "which is obtained from my experiments upon digestion is, that vegetable food is more difficult of digestion than animal, and raw animal more difficult than boiled. Connected with the more difficult digestibility of vegetable food, is its longer continuance in the stomach and cæcum than usual. The stomachs of ruminating animals are often not empty after five days fasting; in the rabbit much food is still to be found in the stomach after a fast of 24 or 36 hours; and in carnivorous animals, pieces of potatoe and carrot that were devoured at the same time with meat, may be found still unaltered, although the meat be long ago digested. Even bread is less digestible than meat, though of all vegetable food the easiest.

"It may be asked, how the longer continuance of vegetable food in the stomach is possible, as the peristaltic motion is as strong in herbivorous and omnivorous, as in carnivorous animals. An explanation of this is afforded by the different forms of the stomach. In all herbivorous animals the small curvature of the stomach is very small, and the œsophagus is inserted near to the pylorus, while the fundus, on the other side of the œsophagus, is very much extended. The large curvature, which includes the whole of the fundus, is, on the contrary, much more developed; and the pylorus itself is short in proportion to the fundus.

"In carnivorous animals, on the other hand, the small curvature of the stomach is more developed in proportion to the large; the œsophagus is inserted nearly at the fundus, and the pylorus is, in proportion to the fundus, longer than in the herbivorous class.

"If, now, the nature of the peristaltic motion of the stomach be more nearly considered, it will be seen to be very different in carnivorous and herbivorous animals. The motion of the stomach may be compared to the peristaltic motion of the intestines, since it consists, like that, of alternate contractions and expansions of the circular and longitudinal muscular fibres. The only difference is, that in the intestines this motion is on all sides uniform, whilst in the stomach the motion of the small curvature is less the shorter it is, and, on the contrary, the more extensive the large curvature is, the greater will be its peristaltic motion, so that in herbivorous animals, the motion of the small curvature may disappear almost entirely. The difference between the peristaltic motion of the stomach and that of the intestines, therefore, will be the greater the more the small curvature is shortened in proportion to the large, and the more it is expanded; and, therefore, at the same time, the pylorus lengthened, the more they will resemble each other; for the pylorus itself has nearly one uniform expansion of its muscular fibres, and, in consequence, its motion, like that of the intestines, is on all sides uniform.

"The motion of the stomach may, accordingly, be considered as defective, since it takes place only upon the large curvature, while the small is nearly motionless. It is evident that this partial motion will be greatest in herbivorous animals, and that in the larger proportional development of the small curvature, and lenthening of the pylorus of carnivorous animals, the motion of the stomach will be more similar to that of the intestines.

"The different kinds of motion in the stomach have a very different influence upon the motion of its contents. In herbivorous animals, when the large curvature is chiefly put in motion, the food is moved only along the course of the larger, and at the smaller is nearly motionless. If, now, the food, as is generally the case in those animals—for instance, in the rabbit—as long as it remains at the fundus, forms one consistent mass, it cannot otherwise happen, than that by the continued partial forward movement of this mass by the large curvature, it will revolve upon its axis in the direction of the peristaltic motion, and be pressed into a ball which takes the form of the floor of the stomach. All the contents, therefore, will not be moved forward towards the pylorus, but only that part which is changed into chyle at the surface of the ball, while the remaining part will continue to turn round in the fundus of the stomach. In this way the food in the stomach of herbivorous animals cannot go over into the intestines before it is quite dissolved, and this motion of the stomach is perfectly correspondent with the long continuance of the food in that organ. It is, however, very different in carnivorous animals. There the motion of the stomach is, corresponding with the quicker digestion, so arranged, that the rotatory motion of the food takes place either not at all, or at most but imperfectly; for the food at the small curvature is moved, although slowly, forwards towards the pylorus, and, therefore, put into a more progressive motion.

"The reason, therefore, why the food continues longer in the stomach of herbivorous animals is, that through the peculiar structure and motion of the stomach it is set into a rotatory motion, whereas the progressive motion into which the stomach of carnivorous animals sets its contents, is correspondent with its quicker digestion.

"That the omnivorous class stands in the middle between these two is self-evident.

"It appears of importance to remark, that in man, in the different states of health, the one or the other kind of motion may preponderate, and that the stomach may accordingly develop itself proportionately to the larger quantity of vegetable or animal food. The fundus will extend itself when the vegetable diet preponderates, as may be seen in old dogs which have always been fed upon vegetables. In this case the digestion will proceed more slowly, and through the rotatory motion the food will be kept longer in the stomach; in the opposite case, on the contrary, the food will be soon removed from the stomach by its preponderating progressive motion. If, now, in the usual formation of the stomach of man, indigestible vegetable food be taken, it will, like digested animal substance, be quickly removed from the stomach into the intestines, and thereby produce a disturbance of the cæcal digestion.

"I now proceed to speak of the act of vomiting, in the different forms of the stomach. The anti-peristaltic motion of the stomach bears the same relation, in the different forms of the stomach of carnivorous and herbivorous animals, to the evacuation of the food by vomiting, as the peristaltic motion bears to its further progress through the pylorus into the intestine. All those herbivorous animals in which a rotatory motion of the contents of the stomach takes place, either do not vomit at all, or with great difficulty; for the food is not urged by the anti-peristaltic motion towards the œsophagus, but merely set into a rotatory motion in the opposite direction, so that notwithstanding it be pressed upon all sides, it cannot be driven out. The animal vomits, therefore, with the greater difficulty, in proportion as the stomach is so formed, that its contents are set into a perfect rotatory motion, as in the rabbit, horse, &c. &c. It has been hitherto said, that horses are prevented from vomiting by the spiral valve at the œsophagus; but this valve can no more prevent the regress of the food than its ingress into the stomach, since its action in the anti-peristaltic motion is merely the reverse of that in the peristaltic, viz., that like the cardia, which is without valves, it alternately opens and shuts, and so, at every time of opening, allows the egress as well as the ingress of the food. The reason, therefore, why these animals during nausea cannot vomit, must be sought alone in the peculiar form and motion of the stomach.

"On the other hand, vomiting takes place the more easily in proportion as the stomach is lengthened, and is in construction more similar to the intestines, for the food may then be driven towards the openings, either backwards or forwards, without being put into a rotatory motion. Hence, dogs and all carnivorous animals vomit without difficulty, and even in man this difference is remarkable, for children, on account of the similarity of their stomach to that of carnivorous animals, vomit easily, while, on the contrary, to older people, whose stomach resembles that of the herbivorous class, this process is more difficult.

"Hillifield (*Experimenta Quædam de Venenis*, Goettingen, p. 50) was already acquainted with the fact, that rabbits could not vomit. I have myself likewise endeavoured to excite vomiting in a rabbit by a dose of two grains of emetic tartar. A quarter of an hour after nausea was produced, together with great disquietude and anxiety; however the animal did not vomit. I have found also, by a similar experiment, that the guinea-pig, which has the same kind of stomach. is not capable of this process. These phenomena do not contradict the experiments of Magendie upon the action of the abdominal muscles during vomiting: for where vomiting is possible, the motion of the stomach gives merely the direction to its contents in which they are to be thrown out; vomiting itself is produced by the action of the diaphragm and abdominal muscles.

"The rumination of ruminating animals appears to take place so, that the food in the first stomach is not at all moved; or at least not in a rotatory manner, for it is generally too fluid, or too little consistent, to be able to form a round ball. However, in this form, small quantities of the chymy mass are more easily pressed into the œsophagus by the action of the diaphragm and abdominal muscles. This process has been ably explained by M. Flourens.

"I must now speak of diseased excitement of the motion of the stomach. There is another circumstance which causes a morbidly quick motion of the food from the stomach into the intestines, namely, too great fluidity. We have already seen, that the food corresponding to its time of continuance in the stomach, becomes more or less consistent. This happens, also, with liquid articles of food, for milk is changed in the stomach of calves into a cheesy lump of almost stony hardness, from which all the whey has disappeared. In milk this is caused, evidently, by its speedy coagulation, and the absorption of the whey by the vessels of the stomach; and in other liquids which are not coagulable, by the simple absorption of their fluidity. It must not be supposed that watery food is in itself hurtful to the digestion, for its fluidity is absorbed and the solid matter remains behind. However, there are fluids which, from their stimulating or indigestible qualities, either always, or only under certain circumstances, quicken the motion of the stomach so much, that the fluidity cannot be absorbed before it, as well as the solid parts, are removed from the stomach without being digested. This, it appears to me, is the cause of the prejudicial effects of milk, raw eggs, coffee, &c., which I have observed partly upon myself and partly upon animals.

"Milk, particularly in weak stomachs, quickens the motion of that organ so much, that even before it is coagulated it disappears through the pylorus. The digestion of the food with which the milk is taken will hereby be disturbed, and as it goes mostly undigested into the intestines, will also cause a dsiturbance of he cæcal digestion, which will be followed by diarrhœa. The action of beer, particularly only half fermented beer, is similar.

"The same is the case with the raw yolk of eggs, the nourishing properties of which I do not dispute, but merely maintain, that in weak stomachs it is hardly digestible. Hard-boiled eggs are more easily digested than raw, as they do not excite the motion of the intestines.

"The effects of cold water are also similar. However, here it is not the water, but merely the cold which improperly affects the motion of the stomach, for in weak stomachs hot water always produces the most beneficial effects.

"Of all fluids, the prejudicial action of coffee is the greatest. The carbonisation of part of its oil, during roasting, appears to be the cause of this property. It is not merely itself indigestible, but also makes all other food, with which it is taken, likewise indigestible. From its stimulating properties the motion of the stomach is so greatly increased, that the whole contents disappear from the stomach before they are digested. This, in the healthy state, and when the stomach is overcharged, may relieve those complaints which are mechanically produced by the distension of the stomach, and it has therefore been generally thought that coffee promotes digestion. However, in fact, it disturbs it, and can only cause a momentary relief, which is presently followed by a greater evil, namely, a disturbed cæcal digestion. It is evident that coffee taken in the morning, and when the stomach is empty, cannot have this effect, for it can only go quickly through the intestines to the already digested excrement, without disturbing the digestion of other food.

"There still remains something which requires illustration, namely, how the stomach of amphibious animals and fishes, which is nearly uniformly extended on all sides, or at least whose small curvature is but little shortened, can detain its contents so long as is usual in these animals. The reason of this may be easily perceived if we consider that the peristaltic motion is here so slow and weak, that it is hardly to be recognised, and has been, indeed, altogether denied by several naturalists.

"The degree of digestibility of meat is different, according to the way in which it is prepared, and also according to the different animals, and the parts from which it is taken.

"Roasted meat is harder to digest than either boiled or raw. Its more difficult digestibility is probably caused by the partial carbonisation and hardening of its muscular fibres, which even after separation from one another go over into the intestines mostly unchanged, while the fibres of boiled meat are entirely dissolved. That according to the degrees of roasting, gradual approximations to boiled meat may take place is self-evident.

"Smoked or salted meat, such as ham, is easier to digest than roasted, and harder than boiled meat. To preserve the meat from spoiling it is saturated with the carbon of the smoke, and is rendered the more indigestible the more it is smoked, and in consequence dried. All substances which are used for the preservation of meat, render it, at the same time, indigestible, as it thereby becomes less soluble, for the most easily decomposable meats are, in general, the most easily digestible. Fat, particularly when surrounded by cellular tissue and thoroughly smoked, is still less digestible than roasted meat.

Cheese, new, as well as old, is nearly as digestible as boiled meat; nevertheless, its peculiar volatile part brings much foreign matter into the blood, which strongly stimulates the secreting organs, and gives rise to various complaints of the bladder. I have found that the flesh of the lower animals is in general harder to digest than that of the higher classes, and the easiest of all is the flesh of the mammalia and birds. Fish, boiled, as well as salted, such as salmon, herring, &c., is harder to digest than the roasted flesh of the mammalia, and roasted and salted fish more so than boiled. The fish of the crustacea, as is shown by my experiments upon crab's-flesh, is still more indigestible and injurious, and is not in the least altered, even when the roasted meat begins to be digested. This explains the fact, that fish and crustacea require very healthy digestive organs to be digested, and very easily excite intermittent fevers in people who are a little disposed to disease. I am acquainted with an example of this in the village of Binnenwalde, near Rheinsberg, where, during autumn, a great many people were afflicted with an intermittent fever every Wednesday and Saturday. Upon further inquiry, it was found, that at this season of the year, the Tuesdays and Fridays were employed in fishing, and that most of the inhabitants ate fish in the evening or on the following day. With the changing of the day for fishing, the day upon which the fever broke out changed also; and when I explained to the people the effects of fish, and the sickly ate no more, the fever became more rare, and at last ceased entirely.

"The violent fits of illness which in many people follow the eating of crabs, &c., and even the poisonous effects of certain fishes, appear to arise from their general indigestibility.

"Oysters are the only exception to the indigestibility of the flesh of the lower animals. Nevertheless, that there is a certain similarity between them and fish, is shown by their frequent poisonous effects. It is also not known whether the oysters are really quickly digested, or whether, from an excitement of the motion of the stomach, they go speedily through the intestines, either partly or wholly unchanged. This is the more probable, as in many people they are apt to produce diarrhœa. The chyme also of oysters is much less acid than that of meat.

"The boiled flesh of fowls and pigeons I found was easier to digest than boiled beef or veal. In general, the flesh of young animals, especially veal, appears to excite the motion of the stomach, and by causing diarrhœa, to render its digestion less perfect than that of the flesh of full-grown animals. I have not observed any difference in the digestibility of the lean parts of mutton, pork, or beef, although pork and mutton are generally less digestible, on account of the larger quantity of fat which they contain.

"In all these different sorts of meat I have observed that their digestibility is much promoted by a perfect extinction of their living principle, by allowing them to hang in the air more or less time, according to the season of the year. I have often seen in cats and dogs, that the flesh of old animals, boiled perfectly fresh, is as indigestible as when eaten raw, and in man also with a weak digestion, the same difference in the digestibility of fresh and stale meat may be observed. The flesh of domestic animals, by becoming a little stale, is more easily digested, and is then also more digestible than that of wild animals. Among these latter, venison and wild boar are hardest to digest.

"In general, it may be inferred from my observations, that it is of great importance to establish a difference between the digestibility and nourishing properties of food, since very nutritious but indigestible substances, such as smoked and roasted meat; are often more injurious than such as are less nutritious but easily digested, as the finer vegetables, spinach, asparagus, &c.

"On the other hand, we must not determine upon the digestibility, or at least the nourishing properties of food, from the feeling of facility with which it is removed from the stomach. All those kinds of food which, by strongly exciting the motion of the stomach, go quickly into the intestines, may be easily borne by the stomach, but are very imperfectly dissolved, and at last are followed by all the consequences of disturbed digestion. They afford the body, therefore, only a very small quantity of digestible matter, and are again excreted mostly unchanged. On the contrary, there are other kinds of food which, on account of their long continuance in the stomach, appear for the moment to be hard to digest, yet are nevertheless perfectly digestible, and never followed by bad consequences upon passing into the rest of the intestines.

"There are, therefore, really indigestible substances which, under certain circumstances, and taken in moderate quantity, are easily borne by the stomach, for instance, oysters, milk, eggs, coffee, &c.; and there are also substances easily digestible, which sometimes appear to be less easily borne by the stomach, as is sometimes the case with boiled meat, particularly when taken in large quantities, which, on account of their perfect chymification, always remain longer in the stomach."—*Lancet, Nov.* 16, 1839.

5. *On the Preservation of Subjects for Anatomical Purposes.* By B. G. Babington, M. D. and G. O. Rees, M. D.—The difficulty which has existed in supplying the medical schools of London with subjects for dissection has made it an object of much importance to discover a method by which human bodies may be preserved from putrefaction. This matter was more particularly brought to our notice last winter, when great inconvenience was felt by the students, not only of Guy's Hospital, but of every school in London, from the insufficient supply of subjects for dissection.

There are, it is true, many methods now in use of preserving animal matter; and the processes of tanning, salting, pickling, drying, smoking, freezing, are so many familiar examples of those methods; but they are all more or less inapplicable to the purposes of anatomical science: thus, tanning, smoking, and salting, wholly alter the appearance and texture of parts; the corrosive action of acids is injurious to the instruments employed in dissection; and immersion in ice, which might possibly be practised, under favourable circumstances, in preserving whole subjects, would, independently of its expense and inconvenience, fail of its effect, when once the student had begun his work. The only antiseptic which is free from the foregoing objections is a solution of alcohol. This, it must be admitted, answers well for museum preparations; but its powers are limited, and its injection into the blood-vessels, even in its most concentrated form, will not materially retard decomposition: moreover, it destroys colour; and when employed in sufficient quantity to admit of the immersion of parts, is too costly for common use.

Some simple experiments of a purely practical nature, which we were induced to institute in consequence of the foregoing considerations, have led to a discovery, the application of which promises to remove one great obstacle to the study of the most important branch of medical education. Our attention was first directed to those chemical substances which were known to coagulate the blood; and we accordingly prepared strong solutions of the following metallic salts; viz. sulphate of zinc, sulphate of iron, and diacetate of lead. We purposely stopped short of the point of saturation, from a belief that the greater density of the fluid, in the case of salts so soluble, would impede its flow on injection. Infusion of galls was also adopted, from its powerful action in precipitating animal matters; and sugar in the form of syrup, being well known to possess preservative qualities, we thought it worth while to make trial of its capabilities. With each of these fluids a rabbit was injected, from the aorta; and another rabbit, killed at the same time as those which were made the subject of experiment, was kept, for the sake of comparison. They were all exposed to the air, in an open court; being merely protected from the weather by enclosure in a wicker basket, loosely covered with oil-cloth. At the end of three weeks, they had become putrid; and we could not perceive that, in any one instance, decomposition had been materially arrested. We were aware that arsenic and the bichloride of mercury both possessed considerable antiseptic powers. A solution of the former had indeed been tried with success at Guy's Hospital, in the previous year; but the poisonous qualities of these substances rendered them, in our opinion, as well as in that of others more nearly interested in their employment, decidedly objectionable.

The total failure of our attempts had nearly discouraged us from proceeding further, when it occurred to us that the preservative powers which exist in certain hydrocarbonous fluids offered some probability that they might be turned to account in the prosecution of our object. Creosote and pyroxylic spirit more especially attracted our attention; and as the former was too expensive to admit of its having been advantageously used alone, we combined it with thrice its bulk of solution of gum-arabic. Two rabbits were injected; the one with pyroxylic spirit, the other with this mixture; and exposed to air, with protection from the weather, precisely in the same manner as was practised in the former experiments. At the end of two months, from the 30th of November, when the injection was performed, these rabbits were examined at Guy's Hospital, and declared by all who saw them opened, to be as perfectly free from putridity, and as fit for all the purposes of dissection, as on the day when they were killed. It should be stated, that in these instances, as well as in the experiment with infusion of galls, a portion of fluid was injected *per anum.*

Having thus far perfectly succeeded, we resolved to obtain permission from the hospital authorities to make a direct experiment on the human subject, as soon as the weather became warm enough to test our method with sufficient severity. In the course of the spring, we were permitted to avail ourselves of the following opportunity:

On the 15th of May last, a convict at Woolwich, 23 years of age, died of inflammation of the bowels; and on the 18th, his body was sent, by order of the inspector of anatomy, to Guy's Hospital, for dissection. It was neither œdematous, nor in a state of decomposition; and although the integument was somewhat fat, it was, upon the whole, in a fair condition for anatomical purposes. On the 21st, a gallon of pyroxylic spirit was injected into the aorta; and the body was placed in a water-tight shell, or trough, made of slate, and loosely covered with a wooden lid. This trough was deposited in a cellar, the stone floor of which was about two feet below the surface of the ground. On the 29th the lid was removed, for the first time, and the body was found to be perfectly fresh. On this occasion, the flesh of the extremities was remarked to have become somewhat firmer than when the injection was first made. From the 29th of May to the 12th of June, the subject was examined, by removing the lid of the trough every two or three days; and no change was perceptible until the latter date. At that time the only sign of alteration was the appearance of two or three brown streaks—evidently veins—on the inside of the thighs; and a separation of the cuticle of the hands from the true skin, which began to assume a greenish hue. Every other part of the body was perfectly preserved, and of natural colour. There was no putrid odour on opening the lid of the trough, but the characteristic smell of the pyroxylic spirit was in some measure passing off. An incision into the middle of the right thigh, such as would be made in operating for popliteal aneurism, showed that the fat, muscles, blood-vessels, and nerves, were in a complete state of preservation. It should be observed, that ever since the injection of the subject the weather had been that of established summer; and that a second body received from Woolwich, was so decomposed in three days after its arrival as to be totally unfit for dissection. On the last examination, as well as on two or three previous occasions, fluid was observed to occupy the bottom of the trough, and this it was thought advisable to remove: it was likewise determined to throw another quart of pyroxylic spirit into the aorta.

On the 24th of June, the body was removed to the dissecting-room, and placed on the table, for the purpose of being thoroughly dissected. With the exception of a greenish appearance on the outer part of the left thigh, and the brown streaks already mentioned, it appeared, when brought into the light, perfectly preserved. The skin on the back of the hands, instead of putrefying, had dried, and become transparent; while the greenness of the left thigh proved, on incision, to be quite superficial. The dissection was undertaken by eight gentlemen, and completed by the 13th of July; and it is testified by them all that every anatomical purpose was as fully answered as if the subject had been quite recent. The various parts, on being laid open, were of natural colour and of firm texture. The tendons and ligaments were silvery and white, and the nerves had lost none of their tenacity. The pectoral muscles alone formed an exception to the natural colour which was elsewhere maintained: this appeared to be attributable to the macerating effects of a wetted cloth that had been laid upon the breast, to prevent evaporation through the aperture by which the injection had been accomplished. The parts which were exposed by dissection gradually dried; changing, in the course of a day or two, to a dark colour, and instead of putrifying, becoming hard. The brain, although it had retained its form, was soft, and semi-putrid, and unfit for demonstration: it must be borne in mind, however, that had the head been opened six days after death—at which period the subject was injected—this probably would have been the case. With the above exception, the viscera were all perfectly preserved: in proof of which, one of the kidneys, appearing, in colour and consistence, quite recent, was removed in the beginning of July, and after maceration in warm water, in the usual manner, was injected with wax. This experiment was made in order to ascertain whether the spirit produced thickening, or any other alteration, in the inner coat of the blood-vessels; which was found not to be the case, as the wax had fully penetrated the tissues of the organ.

Of the gentlemen engaged in the dissection of this subject, one complained that he at first suffered headache from the odour which it exhaled; and some, who were not so engaged, considered this to be more disagreeable than that of putridity. The same opinion is sometimes expressed with respect to the odour of parts that have been macerated in spirit of wine. Some allowance in favour of the pyroxylic spirit should be made on the score of novelty; and since its vapour is not poisonous nor injurious, any more than that of spirit of wine, it is to be presumed that the student would soon become accustomed and reconciled to it. In a first trial, upon the human subject, of the antiseptic powers of this fluid, a natural desire existed, on our parts, of watching its progress, and of noting such changes as might gradually occur. This led to the necessity of opening frequently the lid of the trough: and it has already been remarked, that this by no means accurately fitted the trough itself. The pyroxylic spirit being of a very volatile nature, it is obvious that its preservative qualities were much diminished by this proceeding. It is, therefore, not too much to expect that in an air-tight vessel a subject thus prepared would not exhibit even those superficial changes which took place in this instance, and would be preserved for an indefinite period.

The advantages of employing pyroxylic spirit are, first, its extreme fluidity, in consequence of which it may be thrown into the minutest vessels. Secondly, its freedom from colour. Thirdly, its cheapness; for a gallon is sufficient to inject a full sized subject: and even with the present limited manufacture of it, it is only half the price of alcohol; while it possesses infinitely greater antiseptic powers, and is, in common with that fluid, miscible with water, in all proportions. Fourthly, its innocuous nature, and its freedom from any corrosive action upon steel instruments. We are not aware that there is any material disadvantage in its employment: the odour, it must be admitted, is more or less disagreeable to different individuals, but not so much so, to the generality of persons, as that of the putridity which it serves to prevent. Of this fluid, which must not be confounded with pyro-ligneous acid, or with pyro-acetic spirit, a full account may be found in the "Annals of Philosophy," N. S. viii. 69.—*Guy's Hospital Reports, Oct.* 1839.

PATHOLOGICAL ANATOMY AND GENERAL PATHOLOGY.

6. *Distinct Small Pox, followed in six weeks by an attack of the same disease in its confluent form, and terminating fatally.*—Dr. Renaud has recorded in the *Revue Médicale*, (February, 1839,) the following remarkable example of secondary small pox. A little girl, ætat. 28 months, was seized on the 24th of August, 1838, with small pox of the distinct form, which passed through its regular periods. The pustules left several well-marked cicatrices upon the face and neck.

On the 20th of October following the child suddenly lost its appetite, became dejected and morose, and complained of pains in the loins and hip. On the night of this day, as well as on the two following, she had convulsions. On the evening of the 23d some red spots were observed on different points of the surface; these increased in number and size on the 24th, and on the 25th became so well marked, as to leave no doubt as to the true variolous nature of the eruption. Professor Pelletan was now called in consultation, and assured himself of the reality of the first eruption, the cicatrices of which were still visible, as well as of the nature of the pustules. From this date the symptoms continued to increase, the eruption being confluent upon the face. Death took place on the night of